

Interactive comment on “Carbon Emissions and Removals by Forests: New Estimates 1990–2020” by Francesco N. Tubiello

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General: This is a timely and extremely useful paper. It provides a critical and significant update to previous FAO data. It is critical both to the scientific community who rely heavily on FAO data, and also the policy community in negotiating the Paris Agreement work plan, raising ambition and particularly ahead of the Global Stocktake, as well as in the international review process for inventory data. FAO data forms a key part of evidence for assessment in IPCC reports including the upcoming AR6. The paper is of high quality and high relevance. I strongly urge to publish it as soon as possible. The comments I have are easily dealt with as part of minor revisions.

Specific comments:

Definitions: Some definitions would be helpful earlier in the paper. The paper talks about fluxes due to deforestation (loss of forest area) – Please clarify if this loss of just primary forest area or is it also loss of secondary (regenerated and planted) forest area), I think the latter. Also please clarify that the “forest land” flux includes both increase in forest area due to regeneration and planting, as well as any forest management/degradation or environmental drivers (CO₂ fertilisation, climate change) that in net leads to increasing (or decreasing as in Africa) stock in extant forests. Finally, in the discussion (page 6 lines 170-180) you make it clear that the changing stock in “forest lands” should by definition apply to managed land only, but that some countries also report on unmanaged lands, so this is a mix of anthropogenic and non-anthropogenic leading to an overestimated. This info is helpful to include up front as well as in the limitations section.

Section 2.2 limitations and uncertainty: could you include a comment about legacy effects of deforestation prior to 1990 and move the discussion or some comment on unmanaged lands to here.

Comments by line:

Pg 1 line 23 “Remarkably, the new data also suggest an overall net sink of about -0.7 Gt CO₂ yr⁻¹ during 2011-2015, never reported before.” Do you mean specifically by FAO as the inventories reported a small sink in this period as included in IPCC SRCCL.

Pg 2 line 34 should either refer to the whole IPCC SRCCL, or to chapter 2 Jia et al 2019 where emission estimates are discussed in detail either in addition to or instead of Arneeth et al. .

Pg 2 line 34-36, are these umbers the mean across the three different estimates? If so state this. I think it would be better to give the range, or better still the individual numbers as they differ from each other for different reasons. (may also have the opposite trend?)

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Pg 2 line 51 “showN”

Pg 3 lines 80-84. Its confusing as you say on include on the fie first two of six categories, then list all six, with 1 being AG and BG biomass and 2 being dead wood. But on line 84 you say “we including only ..living biomass”. Please clarify better by listing in the first place what was included, and then what was not included.

Pg 3 line 89-90, you say “two sub-components” but list only one

Pg 5 line 134. I don’t really understand this “Results show that remaining forest land (i.e., net of deforestation)” do you mean “not including deforestation.” See point above re, helpful to define what you include in “forest land” in increase in area of planted and regenerating forests? As well as change in carbon stock on extant secondary (re-growing or replanted) forest areas. Also double checking whether or not it is including change in carbon stock on primary forests, may be pertinent to Africa comment below.

Pg 5 line 144-148. Again as per definition comments above, to double check this is all loss for carbon stock in extant forests, not loss of forest area of secondary forests which would count as deforestation? And is it in all forests, primary and secondary?

Pg 8 line 240. This whole section is lacking a comparison with global models, while you don’t need to go into this in details, I think it is worth highlighting in relation to your concluding sentence. While the NGHGs fine a small net sink, the global models do not (Friedlingstein et al., 2019, subm; SRCCL, Grassi et al 2018) . Then you can also refer to the findings of Grassi et al that this is mostly because in the modelled definition a lot of the sink in extant forests due to increase in carbon stock is considered to be due to the natural response of forests to environmental change, and is not considered to be anthropogenic in the models. I think its important for both science and policy communities to understand this, especially those more familiar with the results of the global carbon project. It only needs to be a short explanatory comment.

Pg9 line 251. I don’t think you can say “never previously detected with this magnitude”

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because in your figure the NGHGs show a similar magnitude. Can you qualify that its never detected at this magnitude by FAO. Also to note that the global DGVM models do detect a large sink in extant forests in both managed and unmanaged lands, but it is not reported as part of anthropogenic flux. This goes back to the point above. You make it sounds like FAO has discovered a large sink no-one previously knew about, when the inventories and models both report this sink.

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